REALITY AND PROBABILITY IN MARIO BUNGE' S TREATISE

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CONTENTS.

1. INTRODUCTION. THE PROBLEM OF PROBABILITY AND PHYSICAL REALITY.

The problem of the nature of probability laws in physics is a central one from the epistemologic point of view : perhaps the most central one for twentieth century physics, if we consider, on the one hand, the harsh debates to which it has given rise in the recent past (left unconcluded by the protagonists and

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most often by their successors) and the vagueness of the answers which are generally proposed by contemporary physicists when asked about this problem ; and, on the other hand, the perspectives of theoretical physics as it is developing under our eyes in the various domains - and, perhaps above all, in the "frontier" domains such as particle symmetry fields and cosmology, or, differently, dynamics of continuous media, turbulences and chaos, where probability is a - if not the - fundamental concept.

For this reason I have chosen this problem among the many items dealt with in Mario Bunge's voluminous Treatise - although a single and specific one, it has, indeed, a strong link with many other thema of this Summa philosophica - : the rapprochement between probability and physical reality points directly toward the heart of the problem such as Mario Bunge's philosophy is inclined to consider it.

Before any commitment to a particular philosophy, we are faced with two subproblems. The first is brought about at the start by the mere juxtaposition of the words probability and physical reality , and concerns the distance that the idea of probability introduces or, rather, seems to introduce, between physical reality, as it is supposed to exist independently of thought, and that subjective touch which seems inherent to a probability statement. The discussion of the current interpretations of probabilities belongs to this subproblem. The second, and fundamental one, is the following : are probabilities inherent to the formulation of physical laws, are they a part of the mathematical formalism of physical theory ? If that is the case, what are the exact nature and meaning of their being incorporated in physics ? This part of the problem is often hidden under the considerations regarding the interpretation, as if probability were, a priori, of a different nature in physics than other mathematical structures (such as geometry or differential calculus). And as if it were, by itself, the object of an interpretation before any incorporation or use in a physical theory, as if, in fact, probability was just superimposed to physical theory, as something alien to it, but henceforward necessary to express the meaning of physical theory. Hence - in my opinion - the excessive importance given, in the epistemology of contemporary physics, to the problems of interpretation considered from an external - i.e. so-called "philosophical" - point of view. But I shall come to that point further, in my concluding remarks.

On these two points - interpretations, type of mathematization -, Mario Bunge has given, in his Treatise as well as in other circumstances, substantial contributions which I would like to recall and discuss. Let us state, to start, that Mario Bunge's reasoned choice among the various interpretations of the applications of probability helps in clarifying the problem of the epistemological status of the use of probability in physics. Because, first, the clear statements he makes are helpful to avoid ambiguities and confusion - even if one does not adhere to the totality, or the systematicity, of his conclusions. And because, also, he chooses an objective interpretation related with his conception of reality and, in particular, with the assumption that physical theory is able to give an adequate representation of this reality. An assertion which was constantly claimed by Einstein, and which seems to me the only one adequate to the legitimate ambition of natural or "exact" sciences : without it, one is left with a defeatist conception of intellectual commitments in science, and ready to accept the ruling of the cheapest philosophical conceptions. We have everyday indications of such a situation, outside the community of scientists and philosophers of science, as is obvious, but inside as well.

Now I will proceed along the following lines. To set the framework, I shall devote some consideration to the historical background of the use of probability in physics. Then I shall come to the problem of the interpretations of probability as being at the forefront of any discussion of its more specific implications in a given science. Next, I shall devote some consideration to the clarification of the meaning of the use of probabilities in the quantum domain, a problem which sheds some light on the relations between probability and physics in general, but seems to require a more specific treatment in so far as quantum probabilities are at stake. Finally, I will come to the more general, and philosophically fundamental, question of the status of probability with regard to the relations between mathematics and physics: the latter constitutes a problem which has not been exhausted by the debates on physics and geometry, and which might be liable to gain some supplement of colours would probability be thought in somewhat similar terms.

2. SETTING UP THE FRAME : WHAT CAN WE LEARN FROM AN HISTORICAL OVERVIEW OF THE EVOLUTION OF THE IMPLICATION OF PROBABILITY IN PHYSICS.

Probability has started in science with the consideration of games and social events ; although it concerned at this stage, at least in the games of tossing dices and of heads and tails, physical events which occur in nature, it was not through its eventual relation with the theoretical aspects of physical events that it was considered. It has been developed exclusively as a mathematical theory, with direct applications to a variety of factual situations. It appears that the ambiguities of the interpretations required to justify these applications arose at that time (seventeenth and mainly eighteenth centuries), at a period where mathematics were often - if not always - thought through concrete or factual situations. In the enthusiastic impetus of scientific thought which was taking hold of the most diverse fields, it seemed to many scientists that mathematics was an abvious tool the use of which did not need specific justifications : probability, in this respect, did not differ, for a time, from other branches of mathematics. Its further fate has been somewhat different, and it would be a matter of comparative study - both in history of mathematics and in history of physics - to try to understand the differences.

Let us take these as an historical fact, and note that, still in the eighteenth century, those who argued in a demand of legitimacy for such or such applications of the calculus of probability were often considered as conservative : this was the case with d'Alembert in the debate wich opposed him to Daniel Bernoulli about the applications of probability calculus to decisions about risks (for instance in bets, as in the Saint-Peterbourg problem) and to social questions (inoculation, insurances) (Paty 1988 a). Althouh physics as such was set aside from these controversies which dealt with quite different fields (the crucial concept was that of expectation, and far from being clear), it was implicitly present insofar as physical objects such as dices or coins were implied. Consider, as an example, the problem of justifying the equiprobability hypothesis, set forth by d'Alembert (Paty 1987), which has been answered only more than one century later on physico-mathematical grounds by Poincaré (Poincaré 1900). Taking the case of the game of roulette, Poincaré justified the hypothesis of equiprobability by considering explicitly 1) the sharing of the disk into equal sections, and 2) the expression of the frequency as a continuous function of the angle (Reichenbach 1920 a and b).

It is only at the beginning of nineteenth century that probability calculus has entered the field of physics, through the theory of measurements and errors. This important step has been made through Gauss' theory, which, in its turn, had been preceded by the Bayes-Laplace theorem (Bayes 1763, Laplace 1774), a landmark which made these developments possible, and by Laplace's celebrated book, the Théorie analytique des probabilités (Laplace 1812). The philosophy of this peculiar application of probability to physics was already expressed in the Essai philosophique sur les probabilités of the same Laplace (Laplace 1814). Probability entered in physics at a moment when this science was thought as a system endowed with an overwhelming determinism of the mechanical type (see the Système du monde of Laplace again). The only way left to chance in science was man's imperfection in his endeavours to approach nature. The subjective interpretation of probability was, so to speak, epistemologically determined in the context of a (classical) deterministic science.

It is worthwhile to note that, at this first stage of the incorporation of probability into physics, it made sense indeed to speak of the probability of one single event, because probability was precisely employed to complete the determination of such events from an imperfect amount of observational data. Then, probability was not intended to give a frequency distribution : on the contrary, it was the frequency distribution which yielded the probability for one event to be in a given state (of motion, for instance). Hence probability was gaining a specific status in physics : it had to do with the definition of a state. True, probability was related with finite conditions of knowledge more than with objective attributions, and in the case of idealized measurements it could be discarded from fundamental theoretical considerations. But its function, in such a context, had nevertheless an objective aspect which has been somewhat forgotten on the occasion of the next step of the history of the incorporation of probability into physics.

This second important phase occurred through the establishment of statistical mechanics ; as an effect, this establishment had two consequences in opposite directions. The first one was in the direction of a more fundamental implication of probability in physics, as probability was used to understand the meaning of a new concept which founded thermodynamics as a science, i. e. entropy. Probability was no more depending on the problem of experimental observation, but entered theoretical physics itself, through a genuine physico-mathematical construction which was at odds with a simple reduction to mechanics, at least in Botzmann's pioneer work1. It happened in fact to provide the theoretical link between this new science and classical mechanics (and it is only, so to speak, in a further stage that this link was thought after the reduction standards).

The second effect was, in the other direction, of leaving the goal of the determination of single events or states, by simply determining mean values, as the concepts of thermodynamics (volume, pressure, temperature…) were equated through statistical mechanics to mean values of the concepts of mechanics (position, impulsion, etc.). Hence the predominance, as in Laplace's time, of a subjective interpretation, for statistical mechanics has since been viewed, through the spectacles of classical (mechanistic) determinism, as expressing our effective ignorance of all the details of the behaviour of the constituent particles of a given physical (thermodynamical) system. Despite this, as Poincaré pointed it out (Poincaré 1908, p. 69)2, there remains, in statistical mechanics, an important objective factor. For, if one knew all the detailed properties of the constituent particles of, let us say, a gas, one would be led to exactly the same predictions for the thermodynamical system as those provided by statistical mechanics.

It seems to me that one could express this evidence in a fully "objective" manner : statistical mechanics does not so much reflect our ignorance of the detailed behaviour of the subsystems which constitute the system under study, than it provides the theoretical way to select from these the (objective) informations (or physical quantities) which are needed to treat the system thermodynamically. With such a formulation, statistical mechanics would be considered as a physical theory in the ordinary meaning, without any need to speak of our ignorance. It remains nevertheless as an historical fact that the subjective interpretation (referring to our ignorance) has been the predominant one : there is scarcely any doubt that the reason for it was in the widespread idea that all physics (including thermodynamics) should be reduced (at least in principle) to mechanics. But one is led to make a distinction, when considering the historical developments, between statistical mechanics in itself - as formulated for example by Boltzmann - as a proper theory referring to its own objects, and its interpretation through the program of reduction to mechanics (a program to which Gibbs was much more committed). It seems to me that the statement of Poincaré about the objective character of statistical mechanics expresses something of this kind (moreover we know that, despite his fondness of the system of mechanics, Poincaré was far from being a reductionist in physics).

(As we noted it, the notion of a single event had been lost in this kind of application of probability : but it has been lost only because it was not pertinent to the problem under study. Statistical mechanics had not to use this notion and is not incompatible with it. However, people often think in terms of a loss - which results in identifying probability and statistics -, because they think that statistical mechanics is bound to a reduction program.)

Mario Bunge, although he seldom commits himself with historical considerations, conveys something similar when he analyzes the meaning of statistical mechanics. Let us simply observe here en passant that it is not by chance if historical epistemology and "structural" epistemology rejoin themselves : depending of course which are the goals and methods they both define. Il they are to be objective (the reference to reality purports to guarantee this character), their objects should have a strong overlapping and their results should be akin.

In Mario Bunge's terms, it is the concept of chance which gets an epistemological status from statistical mechanics considered in an objective way. Chance in statistical classical mechanics, he states in book 7 of his Treatise , "is neither in the individual system components, nor in the eyes of the (blind) beholder, but in the nature of things" (Bunge 1985, p. 148-154). In other words, statistical mechanics by itself informs us about what chance is. From its objectivity, it generates an objective understanding of chance: according to this understanding, chance is not an ontological randomness inherent to individual things, neither a subjective factor of our limited knowledge. Let us note that these have been two ways of interpreting chance also in quantum mechanics (as we shall see, in quantum mechanics probability has acquired still another meaning).

Let us make one comment here. By this specific description of the concept of chance - it is not chance or randomness that makes probability, but, on the contrary, probability as defined by statistical mechanics entails a theoretical definition of chance -, Mario Bunge points out, as I understand it, that probability, in statistical mechanics, is not reduced to statistics : a distinction he makes explicit in his analyses of the various proposed interpretations. This allows me to complete and correct what I said before, about the absence of the concept of a single event in statistical mechanics. Actually, we can recover it through this statistical- mechanics concept of probability.

More explicitly, Mario Bunge makes the very clarifying - and, at the same time, original - statement that chance, due to this status, is an emergent property , similar in that respect to the concepts of thermodynamics3. Thus appears that key concept of statistical mechanics, thermodynamic probability , defined as the "number of microstates (of the components) compatible with a given macrostate (of the system)", and which appears as "an objective property of the system, namely a measure of the play, leeway, or ‘degeneracy’ of the system components" (hence the proportionality of the entropy of a system with the logarithm of the termodynamic probability of the system) (Bunge 1985, p. 149). Entropy and probability are to be interpreted in the same manner (either objective, either subjective), due to this bound, and this constitutes one more convincing argument set forth by Mario Bunge against any subjective interpretation of probability.

We owe to our author other useful clarifications related to statistical mechanics, such as the fact that the latter "associates irreversibility with entropy increase, without identifying them" : one obvious reason for this non-identification being that there are irreversible processes which are not thermodynamical (Bunge 1985, p. 151). (This is true, but I would object to the examples he takes - introduction of a coin in a piggy bank, electron-positron annihilation into three gamma-rays - that these are submitted to probability laws, although they have no entropy defined, and irreversibility can as well be related to a function of those probabilities. I would add that there are irreversible processes in nature which do not come from low probability reversible ones, such as the time reversal violation in some interactions of elementary particles.)

One more remark about statistical mechanics : Mario Bunge states that it has not succeeded in reducing thermodynamics, but only the kinetic theory of ideal gases and that part of thermostatics which deals with the thermodynamics of systems in equilibrium, and he recalls that "no general and rigorous derivation of the second law of thermodynamics" has been obtained ; reduction has been possible - as it is always the case - only with very simple and unrealistic systems4. Furthermore, he adds, one should include, in any reduction program, the quantum poperties of the components of thermodynamical systems (Bunge 1985, p. 153). This last comment reminds me similar ones made by Einstein, not only when evoking the generalized field concept, but also when speaking of the atomic properties of measurement rules for classical physics. With Mario Bunge, such a comment does not intend to mark the way towards the unifying perspective in one single theory, as it has in Einstein's meaning ; but both of them have in common the consciousness that to any theory there is a strictly delimited domain of application, and that it would have no sense to idealize and extrapolate the properties of this theory ouside the frontiers where its conditions of approximation are no longer valid. In particular, this is a strong view against any hasty reductionnism.

Let us come back to the end of our historical sketch. The following step of the incorporation of probability in physics has been through quantum physics. As we shall comme back to it, let us be brief now. Various interpretations, which include philosophical developments, have accompanied the use which has been extensively made of it, and have resulted in a confusion about the exact significance of probability in the quantum domain. This confusion is due in part to the adoption, by quantum physicists, of the received conception of probability issued from the statistical reduction program. But one can try to see, from the effective use of probability in quantum physics, some of its actual properties, independently of (philosophical) interpretations.

To say the essential, as we can extract it from the facts of quantum physics, the use of probability in the description of quantum systems does not restrict to the expression of statistics. There is an objective significance attached to the description of one single event ; but, experimentally, statistics is still the tool used to obtain this description, as one can see when describing how the diffraction pattern of one single electron, or photon, or neutron, is obtained. We are thus led, with a minimum of "interpretation", to consider that there is a difference, regarding quantum probabilities, between : a) the experimental tool, i.e. probability as employed to read experimental results, from a frequency distribution (this being related with the problem of the status of the observation process, and eventually of the instrumentation vis-à-vis the object system); and, b) the interpretative theoretical device, i.e. probability as employed to express in quantum terms the properties of a system which can be a single one. Both are identified (or, better, confused) by the Copenhagen interpretation or philosophy, for which the meaning of a quantum concept is just the one which is given in terms of observation. Although the practice of quantum physics is somehow getting rid of these obscurities, the status of quantum probability is still not made very clear. We shall further inquire into Mario Bunge's contributions to this problem.

There is another step of the implications of probability into physics, which has undergone recent and important developments, pointing at some new statements about determinism. This step has been made through the dynamics of continuous media, such as fluids (turbulences, chaos, etc.). These problems however are in continuity with situations happening in classical (determinist) physics already described at the beginning of this century, i.e. the problem of determinism without predictivity of dynamical states. They deal with the questions of initial conditions, namely of the arbitrary large amplifications with time of tiny variations : we shall come briefly to this problem later, from a more general point of view.

3.THE PROBLEM OF THE INTERPRETATIONS OF PROBABILITY IN PHYSICS.

Mario Bunge's most important contribution to the epistemology of probability in physics consists in his clarifications of the problem of interpretation. He has given a synthetic article on this subject in 1981 (Bunge 1981), the results of which are integrated in vol. 7 of the Treatise (Bunge 1985, p. 75-95, 148-154, 178-219 ; see also vol. 3, Bunge 1977, p. 185-199). One must add to this that all his numerous works on the epistemology of quantum physics deal also with the problem of interpretation of probability in this particular field, and that his reflexion on the quantum case has been most influencial of his general view of the problem. From a systematic point of view, the problem of interpretation of probability in natural sciences considered in general provides clues for the more specific case, although it does not exhaust it, because probability in quantum physics is related to the interpretation of the latter - as we shall see it. It displays also more directly its relation to the problem of the physical use of a mathematical theory. And the latter problem arises precisely as soon as one thinks about what interpretation is.

For, as Mario Bunge points it out (Bunge 1981), there are five conceptions of probability, one purely mathematical, and four which are related to applications. The pure mathematical theory of probability is a chapter of measure theory, given in its standard form by Kolmogoroff (Kolmogoroff 1933), from which it is made clear that probability theory is independent of any application. As a matter of fact, this theory leaves undefined the two specific notions of probability space, F, and of probability measure, Pr : as such it is a "semi-interpreted calculus". Any application of it needs "a factual interpretation of the calculus" (Bunge 1981, the emphasis is mine). This consideration will allow Bunge to build the only coherent interpretation according to his view - and to the spirit of the Treatise . We shall come to it. Let us state for the moment that this "partial semantic indeterminacy" explains why probability can so easily and fructitiously be applied to a variety of situations : "As long as the probability space a is left uninterpreted, probability has nothing to do with anything extramathematical. […] Any attempts to define the general concept of probability in specific terms, such as those of favorable case, or degree of belief, or relative frequency in a sequence of trials, is bound to fail" (Bunge 1985, p. 87).

Mario Bunge's strategy toward probability thus appears to be the following : to secure the general and rigorous mathematical concept by keeping to it a purely mathematical definition (see Bunge 1977, p. 185-90), then, to state unambiguously the definitions required in view of its various possible applications (Bunge 1977, p. 186-7, Bunge 1985, p. 87-8). As he himself states, the definitions he gives for the latter (Bunge 1985, p. 88) "contain a covert assumption : they assume that the correct interpretation of probability space A is that of a collection of (random) factual items , and that of P(x), for every x in A, is that it quantitates the objective possibility of x. […] In other words , the two definitions [given by Bunge] summarize the objectivist or realistic interpretation of probability" (ibidem . Author's emphasis). This choice of an objective interpretation for the applications of probability (note that it is stated universally, for any field, not only for physics) is related to the idea that probability corresponds to objective processes of determination (for Mario Bunge this idea is nothing but a fact) (ibid ., p. 89).

It is this latter consideration that guides his criticism of the other interpretations which have been proposed by scientists or philosophers, namely the logical, the subjectivist and the empiricist or frequency interpretations. The first is related to the probabilities of inferences of propositions (see its refutation in Bunge 1985, p. 90), the second to beliefs and ignorance (refutation in Bunge 1981, and Bunge 1985, p.91-935). We shall say a little bit more on the third one. But let us notice at this stage - following Bunge' analysis (Bunge 1981) - that the subjective and frequency interpretations have become incompatible with an objective conception of science since the establishment of quantum physics, which has entailed a modification of the relations of science and philosophy. The subjective interpretation was indeed, as Laplacian determinism shows, compatible with an objectivity (see also Bunge 1985, p. 91); since quantum mechanics, it is compatible only with a subjective philosophy of science. Probably everybody will agree with that statement. As for the frequency interpretation, as Bunge states, it is no more acceptable by realists, because of the observation-reality interplay. Statements about this state of things are far reaching, in view of the rationalism-empirism debate.

The frequency interpretation (shared, among other, by von Mises and Reichenbach) is indeed an objective one ; according to it, "probabilies are long run values of relative frequencies of observed events". Then, if we would adopt it, on the one hand, probability would be "related to observation rather than with objective facts" (Bunge 1981); on the other hand, there would be no difference between a frequency for a multiplicity of events and a probability for one single event. Both aspects are inherent to an empirist conception of scientific knowledge. And, indeed, this frequentist interpretation is widespread among scientists - as is empirism as well. To me, it is as difficult to refute this interpretation as it is to refute empirism (but I share with Mario Bunge the consciousness of the falsity of empirism, when one considers how science actually works). For, even when probability is referred to one event, a consistent empirist would object that it is still determined from frequencies - as we have observed it in the preceding section. The argument against this interpretation can thus be provided only by the choice of an appropriate reference not only for physics, but for theory in general (a choice which, for me, is of a philosophical nature). This is indeed what Mario Bunge minds when he observes that "a probability statement does not refer to the same things as the corresponding frequency statement" (Bunge 1981, my emphasis), because they are different, even mathematically : they have not the same reference class, probability belonging to individual facts, and being of a theoretical nature, frequency being collective, and empirical (Bunge 1985, p. 94). In Bunge's opinion, "obliterate the difference between probability and frequency, and the heart of the probability calculus vanishes" (Bunge 1981), notwithstanding the obvious correspondance between them6.

To me, the relation between frequency and probability is exactly of the same kind as the relation between an empirical and a theoretical statement, and it is not the theory in itself which entails the difference of nature between both, but a philosophical conception which rejects induction (indeed, the problem is exactly the same as that of induction : if one infers a value for a probability from a value of frequency, it is not through an identification neither through a pure logical link).

Let us finally come to the interpretation favoured by Mario Bunge's approach. As we have already suggested, this interpretation is so to speak entailed by or inherent to the very construction of the conditions or axioms of application settled by the Treatise . The purity of the mathematical formalism having been secured, one is driven to the application to factual situation, by assigning the up-to-now uninterpreted terms a semantic charge relative to a given domain.

The factual interpretation Bunge assigns to probability calculus is the following : F and Pr (x) must be assigned factual meanings, i. e., F being based on a state space of things, Pr (x) thus becomes the strength of a propensity or tendency the thing has to dwell in state x. In fact this statement makes already clear that one interpretation for the applications is favoured : the propensity one. As Bunge states it, "given the structure of the probability function and the interpretation of its domain F as a set of facts […], the propensity interpretation is the only possible interpretation in factual terms". As a matter of fact, it provides the possibility of assigning a probability to an individual fact x (Bunge 1981; see also Bunge 1985, p. 89).

As for the propensity interpretation, according to which "probability values measure the strength of a tendency or disposition of something to happen", Mario Bunge sees its introduction - around 1914 - in the works of von Smoluchovsky, then of Fréchet, before having been considered again by Popper (Popper 1957) and others - among whom himself. I shall not insist now on the exact meaning of this interpretation, and refer the reader to the various definitions, defenses and illustrations which have been proposed of it. Suffices to say that it is the only one that stands in front of Mario Bunge's requisites, the only one to accord well "with both the mathematical theory of probability and the stochastic theories of contemporary science" (Bunge 1981). The next section will have to consider again this problem.

4. THE CLARIFICATION OF QUANTUM PROBABILITY.

The Treatise deals to a large extent, in its volume 7, on the philosophy of Formal and physical sciences , with quantum physics (theory and concepts), under the significative headings of "Quantons", "Chance", and "Realism and classicism" (Bunge 1985, resp. pp. 165-77, 178-91, 191-219). Indeed, before specifying what is the status of probability in quantum physics, it was necessary to clarify the epistemological status of the things or objects with which quantum physics deals.

These objects are quantons , a word coined by Mario Bunge to express their own specificity, as sui generis entities, free from any tribute to waves and particles, and irreducible to classical concepts. One should here recall the importance of a book such as Philosophy of physics (Bunge 1973), where the concept of quanton is presented. This book stands as one of the strongest defenses and illustrations of critical realism. It has be very influencial7 to show how it is possible to dissociate quantum mechanics as a physical theory from its subjective interpretation, which is of a purely philosophical (if not ideological) nature ; to eliminate the subjectivist matrix which weighs down quantum mechanics ; and to retain only the completely physical theory.

For Bunge, as the confusion about quantum mechanics is of a philosophical nature, it must be fought on a field which in no way is that of physics, but that "of logic, of semantic, of epistemology and of methodology". In this respect, he diagnoses the weakness of quantum mechanics in that this theory is unable to enunciate clearly and unambiguously what is its reference . This seems to me the central problem, and it leads us to considerations which may go far beyond the only case of quantum mechanics (see Paty 1988 c). The objective conception uttered by Bunge puts the observer in its proper place which is not the central one : to be sure quantum theory never speaks of the observer in its formulas and calculations ; let such concepts - Bunge advises us - as observer, observable, knowledge, uncertainty, indeterminism, when used to speak of quantum phenomena, to the metalanguage which we use in our daily practice, but not when dealing with the theory itself : they have not any deeper meaning.

It is only when this is duly clarified that we can have an unambiguous understanding of the exact paper of probability in quantum theory. Indeed, quantum mechanics has a stochastic character. For Bunge, a stochastic theory is acceptable in physics only in an objective and physical interpretation of the probability calculus. Referring again to Poincaré, Smoluchowski, Popper, he states that probabilities must be interpreted not as a measure of our ignorance, but as physical properties (Bunge 1973, 1985). It thus appear essential to formulate the propositions of quantum mechanics in a realist language (a language of properties). In particular, those statements which directly concern probabilities, such as Born's postulate, or Heisenberg inequalities, can be unambiguously formulated as statements on properties. For example, Born's semantic postulate (as Bunge emphazises) about the physical meaning of the state function ya (x,t) can be formulated in terms of "the probability that a be at time t in the region of space comprised between x and x+ Dx" (it is | ya (x,t)|2. Dx), instead of a probability of finding a thing in a given state (Bunge 1985, p. 178). Such objective probability concern as well single quantons or events as ensembles. As to Heisenberg relations, their only property is to represent "the quanton extension in real space or in phase space" (Bunge 1973 ; see the analysis given in Bunge

1985, p. 181-7).

As a matter of fact, quantum mechanics is a theory of a fundamentally stochastic nature, but it is, in Bunge's view, a stochastic theory which enunciates well determined laws relative to probability distribution (Bunge 1973), and he speaks in this respect of "stochastic determinism". The difference between probability and statistics, which we have displayed in the preceding section, is obviously of the upmost importance here, and if the tests are necessarily submitted to statistical methods, "we must not confuse reference with test", and "mistakenly attribute quantum theory a statistical character rather than a probabilistic one" (Bunge 1985, p. 179-80, author's emphasis). There would be much more to say about Bunge's reading of quantum-mechanical statements and concepts in an realist and objective way, recalling for example his proposition to consider wave and particle properties as emergent concepts, or his analysis of measurement theory. As I share the essential of his views, it would be an iddle matter to simply repeat what he has very clearly settled.

I will not however close here the chapter of quantum physics regarding the problem of application of probability, because it is my opinion that we can wonder, at this stage, whether the question is fully answered : is it enough to have clarified the objective use of probability, and are we not to consider that probability in quantum physics gains something of a different nature than in the precedent fields (see our historical sketch above) ? In other words that there is something specific which we could call quantum probability ? To discuss this matter8, I would like to make use of an illuminating paper by the regretted J.M. Jauch.

To J.M. Jauch (Jauch 1974), the subjective interpretation of probability which was possible with classical physics, through the invocation of uncomplete knowledge, is simply useless in quantum mechanics, due to the fact that the processes are not the mean values of infra-processes (from some hidden-variable structure, which has been shown to be absent). Jauch takes the point of view "which holds that probabilities in quantum mechanics are of a fundamental nature deeply rooted in the objective structure of the real world", and for that reason he wants to call them "objective probabilities". For him, an important aspect of this situation is revealed by the difficulties encountered by quantum theory, such as in particular the anomalies noted already in 1932 by E. Wigner in relation to the definition of joint probabilities for certain pairs of random variables (the conjugated ones, q and p, for instance) : Wigner observed that no positive joint distribution exists (Wigner 1932).

Jauch's mentionned work offers an interpretation of this anomaly from the point of view of objective probability. In effect, for him, an anomaly such as Wigner's one "is an indication that the classical probability calculus is not applicable for quantal probabilities. It should therefore be replaced by another, more general calculus, which is specifically adapted to quantal systems". Jauch himself tried to construct such a calculus, taking as bases for it specific concepts such as probability field, random variables and expectation values9.

In this endeavour, he proposes to make a distinction between probability calculus on one side, and probability theory on the other. To the latter belong the applications of the first (calculus) to given situations involving observable phenomena. As to the first, it is nothing more than the mathematical formalism itself, i.e. a branch of mathematical measure theory, with no problem of interpretation, and its basis is Kolmogoroff's theory of 1933. (Note that, apart some slight difference in vocabulary, which might however be of some consequence - calculus for mathematics, vs. theory for physics -, this distinction corresponds to the one we have seen in Bunge's work.)

Jauch emphazises the fact that this particular calculus is so powerful in predicting probability "of actually occurring events", and he observes that little thought has been given to this question : this remark leds us directly to the underlying fundamental problem of the use (the "application") of mathematics -, or, perhaps better, the mathematization of physics. And, indeed, Jauch makes a comparison between this situation and the geometry of physical space, and he states it in the very interesting manner which follows. In the same way as it appears better to leave euclidean geometry for the physical world (although we are not obliged to it), it looks unnatural (due to such anomalies as Wigner's one) to express quantum theory on the background of a classical probability calculus. "So", he writes, "just as the geometry of space-time is determined by physical phenomena in the context of a natural theory, it is my belief that probability calculus is equally determined by certain phenomena in the context of quantum theory".

Let us sketch briefly his reasoning at this stage, concerning the basic concepts of classical probability calculus when considering the physical contexts. The concepts of classical probability are that of measurable space, probability measure, random variable, probability distribution function, and expectation value. A probability calculus appropriate to quantum mechanics would have to generalize, by abandoning some too specific concepts of classical probability calculus. To Jauch, the axiomatic formalism of quantum mechanics (such as the one developped by his Geneva school, assuming the quantal proposition system to be a non-boolean lattice, in relation to the properties of non-commutation of non-compatible observables or variables, see Jauch 1968), this formalism is adequate to the search for such a generalization. The difference between both calculus (classical and quantal) appears when the problem of joint probability distribution of two random variables is studied, and it comes from the non-boolean character of the lattice (this difference appears on the background of a close analogy between both, an analogy which however meets its limit).

I have personally no idea as to how far such a new probability calculus, more adapted to the quantum case, would be suitable, on both theoretical and experimental grounds. But Jauch's suggestion, in its general form, seems to me very rich, in that it offers something more than a mere clarification of what probability calculus is or is not, as it stands. It shows a possible dynamics of the relations of physics and mathematics in this troublesome case of probability applications, which has been so often thought in terms of mere interpretation (and mere interpretation is somehow passive). That is why I shall come to it again in concluding the next section.

5. PROBABILITY AS A CASE OF THE PROBLEM OF RELATIONS BETWEEN MATHEMATICS AND PHYSICS (THE PHYSICAL GEOMETRY ANALOGY).

Let us come back to the way Mario Bunge conceives, in his *Treatise*, probability as a case of the relations of mathematics and physics. In the distinction he makes between conceptual and material existence, Bunge states that to a material thing one assigns the character to change in some respect, and to a mathematical construct one assigns the character of being conceivable consistently . Furthermore, the latter is a fiction, it is not real. Hence the problem to solve is the following one : "How do formal (conceptual) existence relate to real (material, concrete) existence ?" (Bunge 1985, p. 33). Bunge characterizes a construct as an equivalence class of neural processes (i.e. which makes that one thinks of the same construct although through different circumstances, physiological or other), in an objective way (i.e. independent of our way of knowing it or analyzing it). "The notion of class and of relation" [which allow us to speak of equivalence], he writes, "as well as many other concepts, are required to account for reality but they are not themselves real" (Bunge 1985, p. 34). He states clearly that the question of the relation of mathematics to reality is but a specific aspect of a more general question about the relations between ideas and the external world. And he emphasises the risk of delusion to think that every mathematical idea would represent some traits of reality (empirical thesis), or that real things are copies of some mathematical objects (idealist thesis). "Mathematics is ontologically non committal " (Bunge 1985, p. 34), in that its propositions and relations are irrespective of the nature of the elements which are dealt with : "Semantic assumptions are no part of pure mathematics. They are part of factual theories." (*Ibid* ., p. 35). Mathematics does not represent the world.

In the section devoted to "Applications of mathematics" (Bunge 1985, p. 75 sq.), Bunge observes that we are equipped "with a rich fund of formal ideas, i.e. of ideas that do not describe reality but provide the necessary framework for models that do describe concrete things" . He then draws the interplay between mathematization and interpretation, a description which, to him, is universally valid for all fields of "problems in science, technology or the humanities", and sets "the philosophical problem of elucidating the notions of mathematization and interpretation" (p. 81).

Probability is given as an example of application of mathematics, and we have already discussed the positions about the interpretations required for applications. I would like to come back to the mathematization-interpretation interplay, which seems to me still demanding.

It is my opinion that the relationship between mathematical constructs and physical elements comes from the fact that physical representation (more generally any representation of the real external world) is obtained through symbolic construction : the latter then shares with mathematics some character of fictitious construction, for reasons due to the specific properties of mathematical entities, among which their "organicity" (as Poincaré stated it somewhere), which reveals that fictitious constructions of this kind are not arbitrary : they have an inner coherence displaying them as members of a whole ; this coherence is not given initially to us, it is to be discovered through analytic reasoning, and in this sense we are inclined to speak of a "reality of mathematics" although it is in a sense which is not the same as the reality of the external world (see Paty 1988 b, chap. 9, and 1984). But after all they might be not completely disconnected.

Then, about the "interpretation" of mathematics, in connection with the discussion about geometry and physical reality : I am wondering whether it is not a restriction and a reduction of the problem to speak in terms of "interpretation of a mathematical theory" as the classical philosophical debates have done it. Is it only interpretation which is at stake ? Is it not, more deeply, a construction of a physical theory, with the use of a mathematical tool ? Rightly, Mario Bunge referred to the problem of mathematization and interpretation , as we have noted above. But he insisted, about probabilities, on the interpretation aspect which seems to be the dominant one for him, in relation with his priority on the affirmation of realism.

But the moment of mathematization is important as well - even in view of the affirmation of realism - : for physics is a symbolic construction. Physics is not an interpretation of mathematics. True, it uses an interpreted mathematical theory. What do we mean when we say so ? The mathematically defined physical quantities are connected to each other through the relations of the mathematical theory (or structure). But it is quite another thing than an interpreted mathematics. See, for instance, the difference between electomagnetic (Maxwell) theory and differential calculus. The integration of a mathematical theory (or structure) is only a part of a physical theory. Philosophers of mathematics about geometry and physics have underestimated this point. Because of their conception of physical theory and of mathematics as well.

That is why the proposition made by J.M.Jauch to try and build a new probability calculus adapted to quantum mechanics, in analogy to the non-euclidean geometries which are more adapted to physical space, seems to me most interesting, even if we restrict ourselves to the epistemological point of view. Of course, on purely physical grounds, we are faced to the question : would such a construction of a supposedly more adapted mathematical tool be useful in any way to physics ? I let to theorists to debate whether quantum theory would benefit from it, from a purely formal point of view ; and I let also to nowadays - or to-morrow - physicists to decide whether some testable prediction is to be expected out of it.

But let us remember another physical analogy : special relativity, as we know, has two formulations which are strictly equivalent regarding experimental prediction, Lorentz-Poincaré's and Einstein's ones. A theoretical formulation of quantum physics which, including a new probability calculus, would simply reproduce the present quantum theory and its experimental predictions, could still represent as well an important progress, in the same way as Einstein's formulation of special relativity is better than the other one. Let's push the analogy somewhat further, adding to the special relativity the theory of gravitation. Again, we have a first version of a theory which embraces electrodynamics and gravitation (including the refinements on Newton's theory of gravitation, such those undertaken already in 1905 by Poincaré, and further elaborations), which keeps absolute and independent time and space as well as euclidean geometry, and consists of a complicated scheme, with many ad hoc corrections and interpretations. On the other hand we have the special relativity paving the road to the general one (with its non-euclidean geometry) that goes so naturally to the solution.

In analogy, we are confronted to-day with a powerful but complicated scheme, the quantum field approach with its unnatural way to remove infinities, and with its "interpretation" of probability. Can we not think of a new theoretical approach, with specific and more adapted concepts and theoretical frame, the "interpretation" of which would be more straightforward ? Perhaps some new concept of probability would be adequate for such a purpose. To be sure this is no more than a wishful thinking. But it corresponds to a real epistemological question. And, at the same time, it provides the answer to the question one might ask Jauch or the other proponents of non-standard theoretical schemes : does a formulation such as that one have any heuristic value ? It is well probable that very few physicists committed to day with particles, quantum fields and cosmology are interested in any way in these axiomatic or fundamental approaches, because they do not see any heuristic value to these : but this could well be shortsightedness. After all, the deepest problem of physics seems to day to reconcile and unify gravitation and quantum mechanics, i.e. the continuous field and the quantum field, or, more specifically, to deal with quantum fields while keeping some fundamental features of gravitation. A sharper formulation of probability calculus could possibly be a useful step : the above analogy suggests that a radical way to a "natural" formulation may well be to modify the deep mathematical structure. And we know than the deep mathematical structure of quantum field theory is related to the probability calculus.

This pretends to nothing more than a commentary en passant . But it is nevertheless a way to point out an important epistemological aspect of the problem of the applications of probability, as a mathematical structure, to a theoretical physical scheme. We are so used with the classical examples, including the physical geometry one, that we are inclined to dissect the terms of this problem in the received way, which is indeed a statical one with respect to the process of construction through which it has been established. A presently unsolved problem offers to our reflexion the unsecurity of a mere possibility, on the actuality of which we know nothing ; but, on the other hand, it has the advantage of keeping us from any ready-made solution. The stake is far more than a mere interpretation of a mathematical structure : it is the construction of physical theory itself.

This reminds us opportunately that mathematized physical theory is different from a superimposition of a mathematical theory plus an interpretation. It is, indeed, a specific structure of its own, incorporating a mathematical one, but through the use of physical concepts which have been built with a mathematical form, and for this reason submitted to the proper mathematical relations. In a way, mathematics is breathed into physics so that a physical theory be possible. It is physical space which has a specific geometrical structure, or geometrical properties, it is not geometry which would be added physical properties or which should be physically interpreted : this means that physics incorporates mathematics and does not merely use, or apply, it. (In symbolic terms, the relation between a mathematical scheme M and a physical theory P is not M+P as the empirists state it, but MfP, as Poincaré and Einstein thought - with differences among them which I have no room to go into here). å

By putting the problem in this way, I simply want to emphazise the strength of mathematization of physics, which, it seems to me, has been somewhat underestimated in the philosophical debates. In spite of my closeness with many fundamental positions of Mario Bunge, it is my opinion that he also underestimates this constituting aspect of physics (an aspect that makes, for example, that there may well be one geometry for physical space, and not a multiplicity of possible choices, as if among mere tools).

6. ABOUT CONCEPTS AND PHYSICAL THEORIES : THE NETWORK AND THE REFERENCE OF THEORETICAL CONCEPTS.

As we have seen it, statistical mechanics and quantum theory, each in its own manner, define a concept of physical probability the formulation and meaning of which are to be taken from the structure of the theory. Being different, these two theories define different concepts of probability : we have tried before to characterize them, with the help of the clarification of Mario Bunge, and eventually trying to go a step further, in the quantum case, following some illuminating statements of J.M. Jauch.

If we wanted to say in a word what epistemological lesson we learn from this state of thing, concerning our conception of theoretical physics, we could perhaps formulate it as follows. The definition of the relevant probability concept as well as the type of mathematical properties (and structure) of these physical theories are provided from the physical theory alone. Indeed, this is a statement about the meaning of what interpretation is, and it converges with other striking examples related to specific concepts of theories (see for instance, for a similar position about the concept of non-separability in quantum mechanics and its interpretation, Paty 1986).

Now, when evoking above the history of the implications of probability in the various fields of physics, we had set aside for the end a similar remark about the dynamics of continuous media. In this field also we meet with some peculiar properties of probability. This time, the problem at stake is that of the initial conditions, and what the physical variables become from the continuous equations of evolution (amplification with time of tiny variations).

The problem which we meet here is that of determinism of the equations without predictivity of the dynamical variables. It is not my intention to discuss in detail this problem and the peculiar epistemological situation to which we are confronted here. Let me simply suggest that a discussion of what predictivity is would be worthwhile. Can we just equate dynamical predictivity (which concerns predictions about the evolution of the system provided by the equations, which are fully determined in a classical way) and the predictions concerning some specific (space-time) variables ? (We know that the instability of the system makes vanish out any a priori predicted values for these variables, which are distributed at random in a finite, and even short, lapse of time.) Can we not ask ourselves, in such a situation, whether it is the determination of the system which is lacking, or if we are not just in front of a determined property of the system - i.e. a given type of instability ?10 In other words, it is the structure of the theory which describes the system as a whole that is the heart of what theory can afford. Although it is completely determined, there are variables (the determination of which we were asking from ouside the theory) which escape definitely any exact determination : that means that such a determination is not what theory must be asked.

If it is legitimate to ask such a question, we are in face of a change in our deterministic demand : the latter would be no more what the system cannot offer us (considering of course that the theory which describes it is fully adequate). We are no more to impose to the system a demand of an information which it cannot afford. There is a inner logic and self-consistency of the system which is such that it is the system itself that imposes which are the relevant quantities. This state of things is related with the fact that nowadays (I mean in nowadays physics) the description of a system can no more be splitted into the so-called system on one side and a general conceptual frame on the other, in which the first would be embedded. To such a splitting we have been acquainted by the standard conception of mechanics (up to and including its special relativity version), which considered a separation between kinematics (relative to the frame) and dynamics (relative to the system).

But general relativity as well as quantum theory (in its quantum theoretical field version) have tought us that such a separation is by too simple, and can only be approximative. Kinematics is to be affected by dynamics, space (and time) are not absolute concepts (absolute in that sense that they would need a complete specification or determination). In short, the proper physical concepts do not exist from all eternity and being the same for any type of system, or any field, imposed from outside to physical systems, laws and principles. On the contrary, they are to be extracted from the global theoretical scheme in a self-consistent way. This is at least how theoretical physics works, when we try to understand the meaning of its concepts and propositions. This is related to the problem of the reference of the physical theory, on which Mario Bunge has given extensive consideration, notably in his Philosophy of physics 11.

This consideration meets with those to which the other fields of applications of probability have led us, although in this case we have not been in a position to extract a specific content of the concept of probability (further studies would be required). Let us be content here with observing that the analyses of these aspects of the relations of probability versus physical theories have insensibly pushed us to adopt a conception of physics, of its aim and role, of its object, which is quite different from the mechanical-deterministic one, which was still implicit in our epistemological demand.

In fact, we do not any more ask theoretical physics to fill in a program of a "metaphysical" nature, "metaphysical" in that sense that it would impose to the theoretical structure unnecessary constraints. The only program we propose to it is to give an exact and adequate description of physical reality. But, of reality, we have no idea of what it is until we form a theoretical description of it. This description is a network of concepts and relations constructed through mathematical tools specifically adapted to this purpose. (And probabilities, which have been the pretext of these considerations, have shown us how a physical construction operates from a mathematical tool.)

This position (which in itself defines a program, of an epistemological nature much more than a metaphysical one) could be called a metaphysical agosticism toward the epistemological value and content of physical theory. I do not know if Mario Bunge would share exactly my formulation, but I can say that his philosophical work and his epistemological clarifications have been most influencial on it.

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FOOTNOTES.

1) I owe a clear understanding of this aspect of Boltzmann's work to my student Katya Aurany whose doctorate thesis will deal in part with it. Olivier Darrigol have recently given a detailed study of Boltzmann's, Gibbs', Planck's and Einstein's ways in dealing with statistics, which show sthe actual complexity of the problem from an historical point of view (Darrigol 1988).

2) "Il faut donc bien que le hasard soit autre chose que le nom que nous donnons à notre ignorance". For more detailed considerations on the introduction of probability in physics, and above all in quantum theory, see Paty 1988 b, pp. 196-201.

3) The notion of emergence is dealt with in vol. 3 - on ontology - of the Treatise (Bunge 1977, p. 97) : when "recognizing the existence of emergent properties", "unlike holism, we regard emergents as rooted in the properties of the components, hence as explainable in terms of the latter, though not by reduction to them".

4) M. Bunge makes also a brief historical clarification about Boltzmann and Planck achievements, which have not been, contrary to what text books are teaching, to have given a general proof of the second law of thermodynamics nor to have deduced thermodynamics from statistical mechanics : a clarification which shows his awareness of the importance of historical facts.

5) The consideration about the difference between objective indetermination and subjective uncertainty made by Mario Bunge taking the example of the "prisoner's dilemma" (Bunge 1985, p. 93) reminds of d'Alembert's objection against the probability of his time, namely his claim of the necessity to distinguish between probability and uncertainty.

6) Bunge observes that there are other means than frequency to estimate a probability : spectral lines, scattering cross-sections. One can object, however, that these are in fact related to frequencies. But that probability needs not be actualized to be stated, as Bunge states it (Bunge 1985, p. 94), on this I agree without reservation. On the problem of the empirical basis of the statistical theory of probability, see for instance Shushurin 1977.

7) My personal testimony as to this influence on me is in my La matière dérobée (Paty 1988 b, mainly chapter 5).

8) On quantum probability, see Suppes 1961, 1963 and 1966. I use this concept here independently on the idea of quantum logics, for which I agree with Mario Bunge (Bunge 1985, p. 190) and with others (John Stachel in particular) that it is not a convenient solution to quantum mechanics.

9) Among the attempts at solving the problem of joint probability, I would like to quote Mugur-Schächter 1977.

10) The obvious reference here is to R. Thom's work (Thom 1972).

11) See also Bunge 1974. Cf. Paty 1988 c. This consideration leads us also to the problem of theoretical completeness in physics : see Paty 1988 d.

ADDITIONAL REMARKS.

Bunge 1985 (vol 7 Treatise). In particular, mathematics is timeless. Bunge remarks that although mathematics is not based in changing entities (the membership of a set is fix), it allows to represent change (through equations of change). [Let me add my own granum salis : as mathematics is strongly related to logic, this is not surprising : logic obliges to consider immutable objects. But mathematical constructs can be made in such a way that they reproduce change in time : it would be a matter of definition. But this "time" would not be physical, it would be purely mathematical. Shall we not distinguish between constructs and properties ? Mathematics as well as physics use constructs ; these have properties ; physics alone is endowed to justify the properties of its constructs by referring them to "real properties" through theoretico-experimental procedures.] For Bunge, unchangeable mathematical objects are able to represent change, due to the association, to mathematical objects, of semantic assumptions ("correspondence rules"), which specify the properties one wants to be represented by the constructs. [N) Bunge 1985, p. 36.]